Referee's comments on

"Granger causality from the first and second derivatives of atmospheric CO2 to global surface temperature, ENSO and NDVI"

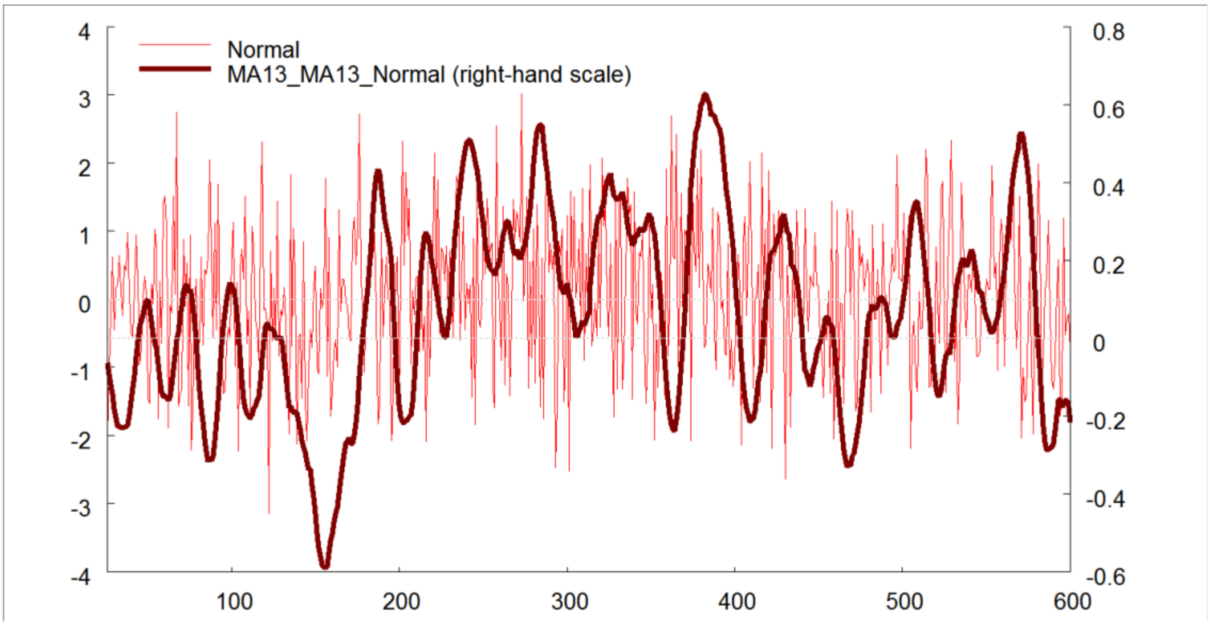
By L. M. W. Leggett and D. A. Ball

This is a rather different paper from the first version on which I have commented for ACP. It is much longer and there is a complete new section dealing with "NDVI", whose relationship with the second difference of $CO_2$ concentration is investigated.

My views are mixed. In principle these results are very interesting. In particular, the findings of various previous studies are re-iterated and confirmed, specifically that there is no relationship, in the relevant historical data, between surface temperature and the level of $CO_2$ concentration in the atmosphere, while a positive relation does exist between temperature and the difference of $CO_2$.

The significance of these facts (if they are facts) can hardly be underestimated, since they contradict the hypothesis on which (what we may call) "global warming alarmism" is predicated. , Evidently, the worst that *continuously* increasing $CO_2$ has done is to raise temperature by a fixed amount, which observation suggest is pretty small. If this pattern continues into the future it is, clearly, not an alarming prospect. Does this finding place the authors among the "97% of scientists who believe in anthropogenic global warming" (as President Obama and others have it) or the other 3%? I'm not clear about this, but these are interesting questions, to be sure.

The authors have developed their methodology with care, and their literature references show that they have a good knowledge of the relevant econometrics and time series literature. One can therefore put some faith in their empirical findings. Nonetheless there are some aspects of their analysis that worry me, in particular the "smoothing" of series by moving averages.




Note that although these are monthly series, smoothing is not the same thing as seasonal adjustment, and its effects on test outcomes are unclear. The attached plot, which I've prepared, shows 600 independent Gaussian drawings, and also the series obtained by applying two successive 13-point moving average transformations to these points. The time series properties of these two series can hardly be treated as equivalent, especially for testing sensitive questions such as phase shifts of one or two periods. In particular, the results of unit root tests are not going to be comparable. Smoothing exaggerates stochastic trends by suppressing high frequency components. I really don't think we can take tests based on these smoothed series at face value.

The authors must first explain coherently why they regard these transformations as necessary to the analysis. At best, they seem to be claiming that the effect is to make a nicer plot, which is hardly adequate. Second, if they convince at the first step they need to show the effects of their transformations by comparing their test results for the smoothed and unsmoothed series.

Something else that concerned me in these causality tests is that although the series in question are being treated as stationary (acceptably in my view) there are still "deterministic" upward drifts in the series. These need to be fitted separately from the higher frequency components, to capture the required "constant conjunction" specified in the definition of causality, and ensure that this is not spurious. (Note that every linear trends is correlated with every other, by construction!) The regressions ought to contain trend terms so that the data are, in effect, de-trended, before correlations are computed. This does not appear to have been done, and it should be.

My third major comment concerns the new section on NDVI. Interesting correlations for sure (subject to the caveats above), but the discussion goes far out on a limb and is, for my taste, unacceptably speculative. First, the series constructed as the difference of standardized $CO_2$ and standardized temperature is a proxy for anything only by a severe stretch of the imagination. Surely, GCMs must (at best) link temperature projections to a particular fraction of projected $CO_2$. (See comment 10 below.) Even if we accept the suggestion that GCM projections are linear in $CO_2$ concentration, the simple difference between $CO_2$ and temperature may or may not capture (in the "constant conjunction" sense) the true forecast discrepancy. Hence, the correlation with NDVI is either interesting by chance, or spurious. I would need firmer evidence to be convinced. The discussion in Section 5 reads like off-the-cuff theorising of the most casual sort. Of course, there is ample evidence, supported by sound theory, for the hypothesis that higher $CO_2$ concentrations are "greening" the planet. To that extent, the authors have a good point. However, it seems to me that their model (involving the second differences of $CO_2$, etc.) needs to be much more carefully derived and argued than it is at present. It's not good enough to simply report a curious correlation and extrapolate from it a whole theory of the biosphere, This seems like blatant data mining.

My suggestion to the authors is to subtract the section on NDVI, as ample material for a new paper although a good deal of additional work is called for. Then, to redraft the first part of the paper taking note of the various comments offered here.

I recommend in particular that plots of the raw data series are shown in the paper, so that the effects of the authors' manipulations can be judged (and also, ideally, the series be made available for download).

Detailed Comments

1. The paragraph in lines 19-25 on page 8 is incoherent. Please redraft. (There are various other places where the quality of exposition could be improved. Please redraft with careful attention to readability.)
2. Lines 13-21 on page 9 are a reworking of the preceding paragraph. Please delete whichever is the unintended version.

3. (Page 11, lines 26-27). The point about SOI versus ENSO could be better made. Is "more valid" a better reason for the preference than "simpler"? It would be very helpful to readers to give brief formal definitions of both these series. How is ENSO constructed? I don't know.
4. (Page 12, lines 9 and 30) The use of the term "derivative" as a synonym for "difference" is, to this reader, an irritating tic. "Derivative" suggests that the models in question are discrete approximations to continuous time relations, but nowhere are these relations specified or the approximations formalized. Indeed, the tests for Granger causality, of the form given, could not be formalized at all in a continuous time framework! Let's be clear that the models presented here are explicitly formulated for discrete sequences of observations. Differences, like lags, are an inherent feature of these models, not approximations to anything.
5. (Page 13, lines 7-16) Please see the main discussion above.
6. There are lots of missing references in the paper. See in particular pages 13, lines 30-31, and 14, lines 4-6, but there are others.
7. (Page 15, lines 9-10) Note that BLUE is a property pertaining to the classical (fixed regressor) regression model, which is not appropriate to time series. Autocorrelated disturbances may result in bias when the model includes lagged endogenous variables among the regressors.
8. (Page 18) The discussion of the "I(*d*)" categorization of series on this page is totally muddled. Beenstock et al. find temperature to be I(1) and $CO_2$ (level) to be I(2). Please redraft with care.
9. The application of the Toda-Yamamoto result is most interesting, but it needs to be seen in context. These authors propose tests for a VAR in levels with an unknown number of unit roots. However, please note that in such a model, Granger causality of an I(1) series by an I(2) series is ruled out by construction. A model generating variables with different orders of integration can only embody long-run relations between variables transformed to have the *same* orders of integration: in particular, between the level of an I(1) and the differences of an I(2), or between the level of an I(0) and the differences of an I(1)). (To verify this statement, consider the VAR $A(L)x_t = u_t$ and verify the properties that $A(L)$ must satisfy to ensure that $A(L)^{-1}$ contains different powers of the factor $1-L$ appearing in different rows.) The outcome of the reported test is inevitable, given the other reported results. I guess it does not harm to report it, but with suitable caveats.
10. (Page 27, lines 11-13) The regression of (say) $x-ay$ on *z* is clearly different for different choices of constant *a*. It could be significant (or cointegrated in the nonstationary case) for some value of *a*, and not for others. The case that the projection error of a GCM can be captured as the simple difference of the two standardized series needs to be much more carefully argued.
11. My guess is that "the APCD paper" referred to in Page 30, line 20, and elsewhere refers to the first version of the present paper. If so, this needs to be made explicit.